Letters to the Editor (continued)

The total cost per kWh generated in nuclear stations (sum of capital charges and fuel cost) has declined from 1·05d. at the first station to be commissioned, Bradwell, to 0·65d. at the latest stations, namely Dungeness, Sizewell and Oldbury (see Mr. F. H. S. Brown's 27th Parsons Memorial Lecture*), so substantial progress has been made and is likely to continue. If electricity from nuclear stations does become substantially cheaper than that from conventional ones, posterity will be grateful for the sacrifice made by the present generation.—*Editor*]

Marconi, Popov, and the dawn of radiocommunication

Dear Sir—With reference to my article in the March issue of *Electronics and Power*, p. 76, my attention has been called to three errors and an apparent discrepancy in dates, as follows:

- p. 77, second column, line 16: for '1895' read '1859'†
- p. 77, second column, line 15 (and elsewhere): for 'Stefanovich' read 'Stepanovich'
- p. 78, first column, third line from bottom: for '1895' read '1898'
- the difference in the dates for the same demonstration (12th March 1896 on p. 78 and 24th March 1896 on p. 79) is due to the difference in calendar used by the sources of information from which these were obtained. The former is in accordance with the old Julian-style calendar, which was in use in Russia until 1918. To convert to the Gregorian style, which has been in use in Britain since 1752, involves the addition of 12 days.—Yours faithfully,

R. L. SMITH-ROSE
21 Tumblewood Road, Banstead, Surrey
6th March 1964

A reproducible second

Dear Sir—With reference to your editorial entitled 'SI units' in the December 1963 *Journal*, p. 501, I would like to query the new definition of the standard second.

According to the writer, since 1960, by resolution of the Conférence Générale des Poids et Mesures (CGPM), the basic definition of the second of time is 1/31 556 925·9747 of the tropical year for 1900. As I understand the article, the whole purpose of international standardisation of basic units is to obtain precise definitions of the fundamental measurement units, which should be continually and certainly reproducible anywhere in the world. To this end, I cannot see how the year 1900, so meticulously subdivided, can be reproducible. Surely such an important elementary unit as the second should be defined in terms of some natural constant that recurs time and again under a given set of conditions.

If the second were expressed as a precise fraction of, say, the half-life of a given radioactive substance, I could understand the way in which this would form a reliable and constant standard. How the year 1900 was timed to 12 significant figures and, further, what device was used that was capable of recording this fact, I am at a loss to understand.

It may be that I have overlooked an elementary principle of some kind, but I would be grateful for a further explanation of this subject, that my mind may be set at rest.—Yours faithfully,

J. G. DAWSON
11 Norfolk Avenue
Rowanfield, Cheltenham, Glos.
27th February 1964

[Some details of the second, or to give it its full name, the second of ephemeris time, were given in an article by Dr. L. Essen in the June 1963 *Journal*, p. 247. It is true that it is not a very convenient unit, but it was introduced because of the variation in the rate of rotation of the Earth and hence of the mean solar second. Radioactive processes do not provide a useful alternative, but for most purposes an atomic second based on a spectral line of the caesium atom is now used. It has not yet been adopted formally, except on a provisional basis, because other transitions are being studied and may provide a still more precise definition in a few years' time. At present the value of the second used in practice is 9192 631 770 cycles of the caesium hyperfine transition, and it is reproducible to about one part in 10^{11}, or one microsecond per day.—*Editor*]

The ternary computer

Dear Sir—In the February 1964 *Electronics and Power*, p. 36, Dr. Alexander presented the usual 'proof' that theoretically the ternary machine requires less equipment* than a similar-capacity machine of another radix. I feel that the usual argument is misleading, in that the implications of its assumptions are not investigated. In the article we were given (1) $E = Rn$, i.e. a ternary device is 1·5 times the binary device, or (2) $E' = n$, i.e. a ternary device is no more costly, bulky etc. than a binary device.

I prefer to assume that $E_R = D_R n$, where E_R is the amount of equipment* for a machine of radix R; D_R is the amount required per digit, and n is the capacity in digits ($n = \log N/\log R$).

If we take the binary machine as a standard, then, for

$$E_R < E_2$$

we require

$$D_R \log N/\log R < D_2 \log N/\log 2$$

or

$$D_R/D_2 < \log R/\log 2$$

Tabulating $\log R/\log 2$ against R, we have:

R	2	3	4	5	6	7	8	9	10
$\log R/\log 2$	1	1·59	2	2·32	2·59	2·8	3	3·17	3·32

I would suggest that, on most criteria at the present time, a ternary device is more than 1·6 times the amount of the binary device, especially when the interconnecting gates are included. Thus the binary machine will require less equipment* than a ternary machine.

I concede that there is every reason to believe that a ternary device will operate at the same speed as a binary device, but if speed is the only criterion, why stop at ternary? An octal device can easily be obtained from three binary devices, and the Table confirms our suspicion that this is equal to binary on other criteria!

—Yours faithfully,

J. M. ROBINS
119 Heath Road, Holtspur
Beaconsfield, Bucks.
8th February 1964

[Dr. Alexander writes: The alternative minimal solution which Mr. Robins suggests is derived from an interesting approach to the problem: that of the amount of equipment required per digit. It confirms that a quasiternary device built up from basically binary devices is bound to involve a greater amount of equipment, as shown in the article.

I should add that, at Nottingham University, we are rather suspicious of an octal device constructed from three binary devices, the contention being based, we feel, on a false precept.]

The industrial designer

Dear Sir—I really must support Mr. Cobbe (February 1964 *Electronics and Power*, p. 55) in his plea regarding articles of the type published in the December 1963 *Journal*. In the March 1964 issue, p. 92, Mr. Howe does him an injustice in confusing 'articles of the type' with 'articles on industrial design'. Did not Mr. Howe trouble to read Mr. Cobbe's letter?

The self-righteous observation in Mr. Sharp's article that '. . . the industrial designer must . . . start with some aesthetic sense, and this approach separates him from the engineer' is itself sufficient to nauseate any Corporate member whose professional background has enabled him to rise above the dubious status of that of a ham-fisted fitter's mate. Let us not lose sight of the fact that the maxim 'if it looks right it probably is right' originated among the engineering fraternity long before IDs ever existed.—Yours faithfully,

J. M. CRANMER
April Cottage, Sheerwater Avenue
Woodham, Weybridge, Surrey
4th March, 1964

Early British steel-tank rectifiers

Dear Sir—Dr. J. C. Read, in his letter in the February 1964 issue of *Electronics and Power*, p. 55, suggests that it would be interesting to hear more about early British steel-tank-rectifier activities.

As far as GEC were concerned, between 1923 and 1926 a 6-anode steel-tank rectifier was built and tested. This rectifier had porcelain anode and cathode seals, and the anode shields were insulated from the main tank. As a result of this work, in 1927 a completely new design was evolved for a 1000A 750V rectifier using a novel type of 'vitric' seal. This type of seal, which a few years later was used in the pioneering of the pumpless steel-tank rectifier, is still used today. Furthermore, the idea that it was essential to insulate the anode shield from the main rectifier tank was abandoned. The design based on solidly bolting the

* BROWN, F. H. S.: 'The duty and development of modern power station plant', *Proc. Inst. Mech. Engrs*, 1963, 177, p. 1133

† A printer's error.—*Editor*

* The arguments are not changed whatever criterion is being considered—cost, size etc.

longer-than-predicted time after the closest approach?

A further consequence of Dr. Stephenson's theory would be that the known *apparent* positions and motions of the bodies of the solar system would be interpreted as representing slightly different *absolute* positions and motions, compared with present theory. Would the use of these alternative positions and motions show a better reconciliation between the observed perturbations of, say, Venus's orbit and the calculated perturbations derived from the gravitational effects of the other bodies of the solar system?—Yours faithfully,

R. D. KENDON
20 Imperial Avenue, Gelding, Nottingham
19th March 1964

A new deal for vector diagrams

Dear Sir—Mr. Scroggie in his article in the February 1964 issue of *Electronics and Power*, p. 47, states that 'there must be satisfactory conventions to relate the vector diagram to the circuit diagram', from which one may deduce that 'vector diagrams are supposed to show the electrical conditions in a.c. circuits'. This I feel is a misinterpretation of the basic purpose of a vector diagram.

The electrical engineer is introduced initially to the concept of an alternating quantity by means of sinusoidal curves and trigonometric expressions of the type $X = X_m \sin(\omega t + \phi)$. It is the difficulty experienced in the addition, subtraction and multiplication of this kind of expression that has produced a graphical method of achieving the same results—the vector diagram. Thus the basic purpose of a vector diagram is to provide a graphical representation of a sinusoid that will be easier to manipulate and will yield a more ready interpretation than the corresponding trigonometric expressions. In order to achieve this end, the convention established must be mathematically rigid, in the sense that it is exact, sufficient and complete, without omission or excess of significance.

Such a definition has, I submit, been given in my article in the June 1959 issue of the *Bulletin of Electrical Engineering Education*, p. 55.

In relation to Mr. Scroggie's article, I would take issue on a few specific points:

- Why is he so much concerned with ascribing a specific polarity to a voltage which he knows to be alternating? Here surely is an excess of significance that leads to confusion. The information sought is already included, without the use of lettering, in vector diagrams following the convention described in the article already alluded to.
- How is the length of his vector determined, and why are some vectors drawn at right-angles to others?
- Do not the vector diagrams of Fig. 2 merely illustrate that vectors are associative and commutative? They are only likely to lead to confusion if improperly explained; they do not appear to be basically inconsistent among themselves.
- If, in Fig. 4, V_{dc} represents a maximum positive voltage across the inductor L, does V_{cb} represent the maximum positive voltage across R? If the answer to this question is 'yes', does this imply that the maximum voltage across L occurs at the same time as that across R? If not, what does V_{cb} represent?—Yours faithfully,

D. CONNELLY
51 Oakridge Avenue, Radlett, Herts.
15th March 1964

The industrial designer

Dear Sir—With regard to Mr. Cranmer's letter in the April 1964 issue of *E & P*, p. 128, why should it be 'self-righteous' of me to assume that some engineers have no aesthetic sense? A brilliant electronics engineer may have to obtain assistance from a mechanical engineer to house his circuits, just as the civil engineer or architect needs the electrical engineer to assist in lighting his buildings.

Design today is such a complex subject that it is necessarily a team job, and I set out to show the place of the industrial designer in that team. I am quite sure that other engineers could, as both Mr. Cobbe (February *E & P*, p. 55) and Mr. Cranmer have done, tear quotations out of the context of my article to make glib points like Mr. Cranmer's—'if it looks right it probably is right' (since when did this useless but widespread maxim get the qualification?).

I refer your two somewhat prejudiced contributors, who wish to keep such articles as mine from your pages, to a far better authority than myself: 'Engineering design' (usually known as the Feilden Report), Chapter 2, Sections 8 and 9.—Yours faithfully,

P. E. M. SHARP
27 Kings Drive, Wembley, Middx.
5th April 1964

* * *

Dear Sir—Mr. Howe, in his letter in *Electronics and Power*, March 1964, p. 92, has confused my criticism of a mode of presentation of a subject with criticism of that subject.

My letter in the February 1964 issue, p. 55, deplored articles of a type that can bring discredit to their subjects. This is far removed from criticism of the particular subject, which happened to be 'industrial designers', about whom and about whose work I would certainly be happy to read more, provided that it is presented in a restrained and objective manner, as is normally the custom in your journal.

Yours faithfully,
D. W. R. COBBE
18 Olivia Drive, Leigh on Sea, Essex
10th March 1964

Marconi, Popov, and the dawn of radiocommunication

Dear Sir—I was interested in reading of the early work on radio of Marconi (March 1964 *Electronics and Power*, p. 76), whose memory we respect, as will be seen in the following. After the foundation work by David Hughes, who first sent signals between two fixed points along Portland Place, Marconi, with the collaboration of the Engineer-in-Chief and Electrician of the Post Office, Sir William Preece, came to Penarth near Cardiff and on the Head fixed up a transmitter facing Sully Island. A receiver was built at Sully, and between them they made the first radio transmission across water. A memorial recording this achievement was put up on Penarth Head.—Yours faithfully,

B. A. CRONIN
33 Tidenham Road, Ely, Cardiff
9th March 1964

Electrical synthesis of music

Dear Sir—Contained in the March 1964 edition of *E & P*, p. 83, was an article entitled 'Electrical synthesis of music' by Alan Douglas. I am surprised at what he said when referring to electronic organs. To quote:

'Today, the few very large electronic organs of true church or cathedral fidelity use valves exclusively. Smaller types may have valves or transistors. The rotating-generator systems have advanced very little and are in the minority.'

In fact, two of the biggest manufacturers of electronic organs for that kind of use, and that find their way into countless churches, do not use valves exclusively. The rotating-generator system may have advanced little compared with valve generators, and may also be in the minority when taking into account all manufacturers, but this system would, I am sure, be found in two of the three 'biggest and best'.

J. D. CATTERMOLE
12 St. Pancras Green
Kingston, Lewes, Sussex
18th March 1964

* * *

Dear Sir—I was interested to read Mr. Alan Douglas's interesting and instructive article on synthetic musical sound (March 1964 *Electronics and Power*, p. 83). It is no doubt true that no mechanical advances in musical instruments over the past 100 years have affected their quality of sound to any extent, but advances in their manipulation have been numerous.

Let us consider changes made to some of the instruments of the present-day orchestra. The change from three-piston valves to four rotary valves for the French horn is probably the biggest change. The clarinet has had its Boehm key system, as does the flute, the latter being usually made today of metal instead of wood or ebonite.

I should think that wire-wound strings are not yet 100 years old; certainly the screw adjustment for the 'E' violin string is not yet 50 years old. Trombones have 'grown' some rotary valves.

Adolph Sax, mentioned in the article, was also the inventor of the saxhorn, which was the forerunner of several instruments of the brass family.

These are but a few of the improvements made in musical instruments during the past 100 years.—Yours faithfully,

F. R. DUNMAN
'Grasmere', Molesey Road
Walton on Thames, Surrey
6th April 1964

Letters to the Editor (continued)

In my Fig. 4, V_{dc} (a vertically upward line) does, as Mr. Connelly suggests, represent the maximum positive voltage across the inductor L. That being so, his query as to whether V_{cb} (a line pointing horizontally to the right) also represents a maximum positive voltage is surely one that would not unduly tax the most elementary student halfway through lesson 1 in a course on *any* system of vector diagrams. However, on the assumption that the question is for information and not to make some debating point obscure to me, I will reply that, in accordance with accepted convention, it represents a voltage which at the instant shown is zero, being 90° lagging with reference to V_{dc}. We can also see at once, for example, that if the point d in the circuit is earthed, the whole of R_1 is momentarily at maximum positive potential. Information of this kind is not obvious at a glance in any of the many conventional alternatives in Fig. 2, which is another reason for their speedy rejection.

Turning now to Mr. Golding's letter, printed above, I find this shows a complete failure to perceive the simplicity of the new system. He says that, in order to determine the potential of c relative to d at any chosen phase of the e.m.f. (which was the example I chose), neither type of diagram is really satisfactory, since the potentials of c and d need to be determined with respect to a and then subtracted. This may be necessary with other systems, but it certainly is not with mine. All one has to do is see how far c lies above d in the vector diagram. It is as simple as that. At the instant illustrated, in which the e.m.f. reckoned with respect to a is 20° from zero towards positive maximum, c is obviously at positive maximum with respect to d.

Mr. Golding's treatment of the problem suggests that he is assuming that point a is earthed. But there is no information to say so. My vector diagram leaves the matter completely open; it is the only kind to be quite unprejudiced. Others make what may be unwarranted and misleading assumptions implied by their disposition of vectors. Yet the moment an earthing point is known, even though it be a point not actually on the circuit, it can at once be indicated on my vector diagram, and the potential of any and every point in the circuit at any phase of any current or voltage can readily be seen by suitably orientating the diagram. This is a facility far beyond any other type of diagram and becomes more valuable the more complicated the circuit. Yet the diagram is manifestly simpler than any other kind. Does Mr. Golding really think it is open to dispute that a total of six letters is neater than ten letters, two subscript figures, one Greek symbol, one extra line and six arrowheads? The atter would be hard to justify unless they gave more information, but in fact they give less.

The rest of Mr. Golding's objections do not arise. In particular, there is absolutely no need to shift components around (quite inadmissible practically in h.f. circuits) or to modify my vector diagram. The only admissible modification is indication of earth point, when known. Nor, of course, does the problem of finding where collinear vectors begin arise, nor does that of finding a sharp-pointed pen or pencil. Anything that can distinguish one letter of the alphabet from another will do. There are no subscripts on the diagrams, either. There is no problem about point d nor need for dots. And, contrary to what Mr. Golding says, there are no ambiguities.

With regard to the transformer (my Fig. 3), which has been commented on by several correspondents, the rotation in which the coils were drawn, being the same for both, supplied all the necessary information; but if the belt-and-braces men like to add the dot notation to make sure, I would not stand in their way. It is, I think, neater and better established than Mr. Golding's.

Lastly, I have received a letter from Dr. D. Midgley of the Bradford Institute of Technology pointing out that even the one weak point—a difficulty (not insuperable) with current notation in 3-phase circuits—which I disclosed on p. 133 of the April issue, was not so weak as I had feared, as there were at least two other and simpler devices for including them in the system.

So far, all criticisms that have been made of the new system have arisen from misunderstandings of it, owing no doubt to my incompetence as an expositor.—Yours faithfully,

M. G. SCROGGIE

18 Bromley Common, Bromley, Kent
23rd April 1964

Marconi, Popov, and the dawn of radiocommunication

Dear Sir—In his article on the dawn of radiocommunication in the March 1964 issue of *Electronics and Power*, p. 76, Dr. Smith-Rose did not mention the name of the man who invented the coherer and was the first to produce and detect radio signals experimentally. But it is not unusual for an author to forget to name this man. None of the articles I have read on the subject contained any reference to the man whose radio researches preceded the earliest experiments of Hertz, Marconi and Popov.

The name of the forgotten man is David Hughes. He was a Londoner who lived from 1831 to 1900. In 1886 he was the President of the Institution.

In 1879, Prof. David Hughes was engaged upon experiments with his induction balance. Then, because of the accident of a bad contact in his balance, he was led into making a series of experiments that resulted in the generation and detection of radio waves. During the course of these experiments he invented the coherer. He used his coherer, together with a telephone and a battery, to detect radio signals up to distances of 300 yd. The circuit details and the rest of the experimental data will be found in his notebooks, which are now preserved in the British Museum.

Prof. Hughes discussed the results of his experiments in January 1886. It was the occasion of his Presidential Address to the Institution. He showed some wonderful experiments and then outlined his theory of the induction balance he had invented. These results were highly complex, and among his audience there was no one better qualified to understand them than Oliver Heaviside. Heaviside was quick to point out that they provided the first experimental evidence in support of Maxwell's electromagnetic-wave theory. He said that they also indicated the coming of wireless telegraphy.

I think it is a remarkable fact that so many writers on the subject of the inventor of radio should have ignored the work of Hughes. One gets no idea of how far Marconi, or Popov, would have got without the coherer invented by Hughes. And after reading some of their articles one cannot help feeling that the authors have carefully selected their data and then correlated their selection in the light of Marconi's, or Popov's, golden sunset.—Yours truly,

H. J. JOSEPHS

'Homefield', Clover Rise
Tankerton, Whitstable, Kent
16 March 1964

[It is hoped that Dr. Smith-Rose will comment on Prof. Hughes's early contribution to radiocommunication in a later issue—Editor.]

Why 'Associate'?

Dear Sir—The qualification Associate Member is rightly an anachronism. When first used it may have expressed the desired standing of the class of membership intended, but progress would seem to have made the meaning obsolete. Surely the time has arrived for a revision.

May I suggest with Mr. Sayers (October 1963 *Journal*, p. 440) that 'Members' be ranked as 'Fellows', and 'Associate Members' as 'Members'. This classification is in fact used by other scientific bodies, such as the Institute of Navigation.—Yours faithfully,

J. K. V. LEE

Harrow Cottage, Knockholt, Kent
5 March 1964

* * *

Dear Sir—Why do we not drop all this Member, Associate Member etc. nomenclature and call ourselves 'Chartered Electrical Engineer' (C.E.E.) and 'Senior Chartered Electrical Engineer' (S.C.E.E.)? This would have the advantage of giving us one form of description (like Chartered Accountant) and would avoid the confusion that is always created when one series of letters (e.g. M.I.E.E.) changes its meaning.—Yours faithfully,

R. A. LAWS

39 Park Road, Watford, Herts
6th March 1964

Letters to the Editor (continued)

With one exception, all the experiments that have been performed to test this postulate have been vitiated either by the fact that the source was brought to rest before emission without the experimenter's realising it, or by the fact that the light initially emitted from a moving source was allowed to interact with the observer's apparatus, from which it was reradiated with the velocity c that was observed; in this case no information is obtained as to what was the velocity of the light with respect to the observer as it travelled towards him from the source, before interacting with his apparatus. Thus the 'overwhelming evidence' allows us to conclude no more than that the light emitted from a source at rest with respect to the observer has velocity c with respect to the observer, regardless of the common state of motion of observer and source.

The one exception to this is an experiment by Alväger *et al.*,[3] in which the time of flight of γrays between two points was measured, the γrays being obtained from fixed or moving emitters. The authors give their experimental observations, and state that from them they conclude that the invariance postulate is confirmed. However, working from their published data, I conclude that the velocity of light appears to be the vector sum of c and the velocity of the source.]

1 LORENTZ, H. A.: 'The theory of electrons' (Dover, 1952), chap. 5
2 EINSTEIN, A., *et al.*: 'The principle of relativity' (Dover), p. 41
3 ALVÄGER, T., NILSSON, A., KJELLMAN, J.: *Nature*, 23rd March 1963, 197, p. 1191 (letter)

Cosmological models

Dear Sir—The usual explanation of the spectral red shift of stars outside this galaxy is based on an expanding-universe model. It seems that a stationary-universe model, in which mass changes occur, may have been overlooked. The observed red shift could be explained in four simple ways:

(*a*) relative motion between all parts of the universe
(*b*) a 'tiring' of electromagnetic energy with distance
(*c*) an interaction of electromagnetic energy with intergalactic matter, producing an accumulative transverse Doppler shift
(*d*) a uniform increase in the masses of all atomic particles, throughout the universe, with time.

The last two possibilities do not appear to have been previously considered.

The expanding-universe model is based on interpretation (*a*). It is postulated that the distances between all points in the universe are increasing at a rate proportional to their separations. This could arise from either the continuous creation of matter in space, or from the creation of all matter at a single point in time.

A model of the type proposed in (*d*) is a stationary model in which the masses of all atomic particles increase uniformly with time. The observed spectra from distant stars would have been produced by atoms containing electrons of lower mass. The lower energy associated with their radiation would produce an apparent spectral red shift when compared with the spectra of corresponding atoms on the Earth. On the basis of continuous creation of matter, this is a very simple model. Physically, it might be more plausible for matter to condense on existing atomic particles rather than in space. This model is also consistent with the creation of all matter at a single point in time; in this case the condensation of stored energy, centred on existing atomic particles, would lead to their masses increasing. The formation of new galaxies would regularly occur when the local density of intergalactic matter reached a certain critical level.

Gold and Hoyle[1] comment that attempts to explain both the expansion of the universe and the condensation of galaxies must be largely contradictory so long as gravitation is the only force field considered. Electromagnetic generation processes have to be postulated to make the expanding-universe model plausible.

A mass-change model explains both the spectral red shift and the formation of galaxies without further postulates, and thus appears attractive on the grounds of simplicity. Such a model is also directly related to the electromagnetic gravitational model I have previously proposed.[2]—Yours faithfully,

L. M. STEPHENSON
University College London
Gower Street, London WC1
4th May 1964

1 GOLD, T., and HOYLE, F.: 'Cosmic rays and radio waves as a manifestation of a hot universe' *in* 'Symposium on radio astronomy. Paris, 1958', (Stamford University Press, 1959), p. 583.
2 STEPHENSON, L. M.: 'A modified source theory of electromagnetic propagation', *Nature*, 1964, **202**, p. 375

Marconi, Popov, and the dawn of radiocommunication

Dear Sir—I am indebted to Mr. H. J. Josephs for his letter in the June 1964 *E & P*, p. 210, drawing attention to the work of Prof. D. E. Hughes. I have read Prof. Hughes's presidential address delivered on the 28th January 1886 and published in Vol. 15 of the *Journal of the Society of Telegraph-Engineers and Electricians* (as the Institution was then called) together with the discussion, which occupied two further meetings. The address and the discussions were confined to a description of investigations into self and mutual inductance, with particular reference to the phenomena associated with line telegraphy and with the well known Hughes induction balance. There is no record that Oliver Heaviside was present at the meeting, as Mr. Josephs states; so I conclude that his contribution and Hughes's description of the coherer must be contained in the unpublished notebooks at the British Museum.

Prof. J. A. Fleming was, however, present at the meetings mentioned above, and in his classical treatise, 'The principles of electric wave telegraphy and telephony', 2nd edition, 1910, the work of Hughes on coherers is described (pp. 420 and 433); also, on p. 514 of the book, reference is made to unpublished investigations by Hughes 'in which signals were sent' a few hundred yards 'without connecting wires by the aid of a telephone'.

In further reference to Hughes's work on the coherer, also unpublished, Fleming concluded on p. 516: '. . . much remained to be done, which was subsequently done by Hertz and by Marconi, before electric wave wireless telegraphy, in any true sense of the word, could be translated from dream to fact'.

More recently, my attention has been drawn to the issue of the American periodical *Saturday Review* (7th March 1964) containing an article entitled 'The real beginning of radio' by Otis B. Young. This article refers to the experimental work of Mahlon Loomis, who, in a notebook dated 20th February 1864, now in the US Library of Congress, describes experiments carried out six years earlier (1858). Two separate wires, 600ft long, were held up by kites on two mountain peaks, 2000ft high and some 18 miles apart. Each was connected through a galvanometer to a coil of wire in wet ground. It was observed that, when one wire was connected or disconnected, the galvanometer needle in the other wire was deflected. Although it is claimed that Loomis did transmit intelligible messages over a distance of 14 miles in October 1866, and he patented the system of 'wireless' in 1872, he failed to get the support necessary to develop this technique.

In conclusion, it may be said that, from the information given, Loomis, like Hughes, was working within the mutual-induction field between the conductors used in the experiments. It is well known that Sir William Preece of the British Post Office had later used similar techniques in the development of a practical system of communication. But Preece was among the first to appreciate and describe the advantages of Marconi's results in using electric or Hertzian waves of very high frequency.—Yours faithfully,

R. L. SMITH-ROSE
21 Tumblewood Road, Banstead, Surrey
8th June 1964

* * *

Dear Sir—In the June 1964 *E & P*, p. 210, Mr. H. J. Josephs credits Prof. David Hughes with the invention of the coherer and, by inference, implies that Hughes is to be numbered among the real pioneers of radiocommunication. This seems to me to give Hughes far more credit than that to which he is entitled and thereby to deny to Branly and Lodge the honour which is justly theirs.

I certainly would not wish to decry the work in other fields of one who became a President of the Institution—Hughes's work in developing the printing telegraph justly earned him a large fortune—but his contribution towards radiocommunication was altogether incidental and insignificant.

It tends, I always feel, to be a little misleading to describe Hughes in this context as a professor. He was a professor of *music* at Bardstown College, Kentucky, a chair to which he was appointed at the age of only 19, and, although it is true that he subsequently held the chair of natural philosophy for a short while at the same college, he was not a trained scientist or mathematician, as anyone who has ever glanced through his notebooks will quickly realise.

As Mr. Josephs mentions, it was while carrying out experiments on his induction balance during the autumn of 1879 that Hughes was led, by the accident of a bad contact in the circuit of the Bell telephone receiver he was using as a detector, to investigate certain strange effects which seemed to arise from what, in the language of the day, was known as the 'extra current' which arose in an inductive circuit when the current was interrupted. It was not, of course, generally realised in those days that the so-called extra current was an oscillatory transient of high frequency.

Hughes carried out many experiments during the closing months of 1879, and he found that the inclusion in the circuit of one of his many forms of experimental microphone enabled him to detect the occurrence of an extra current, even when his detecting circuit was removed to a considerable distance. In later years, it became evident that Hughes had, in fact, discovered a form of the device to which Oliver Lodge gave the name 'coherer' in 1894, and that Hughes was really detecting electromagnetic oscillations radiating from his primary circuit. He was able to detect the sounds of the 'extra currents' at quite considerable distances, and 'after trying successfully all distances allowed in my residence in Portland Street', he wrote in 1899, 'my usual method was to put the transmitter in operation and walk up and down Great Portland Street with the receiver in my hand, with the telephone to my ear. The sounds seem to slightly increase for a distance of 60 yards and then gradually diminish, until at 500 yards I could no longer with certainty hear the transmitted signals.'

Hughes demonstrated his experiments to a number of the distinguished scientists of the day, including William Preece and Sir William Crookes, but it was the demonstration to Mr. William Spottiswoode, then President of the Royal Society, Prof. Huxley and Sir George Stokes on the 20th February 1880 that was to have such unfortunate consequences for Hughes and for the history of radiocommunication. Towards the end of a lengthy demonstration at which they had evidently been initially impressed with his results, Prof. Stokes and his colleagues expressed the view that all the results could be explained by simple induction, and they refused to accept Hughes's opinion that they were due to 'conduction' through the air.

It is easy to be wise after the event, and there can be no doubt that Hughes was, in fact, generating and detecting electromagnetic waves of high frequency. It is equally certain that Hughes himself, whose scientific knowledge was very limited, had no real understanding of the phenomena he was demonstrating. In a letter to J. J. Fahie written 20 years later in which he complains bitterly of the discouraging remarks and treatment he had received from Stokes, Spottiswoode and Huxley, Hughes refers repeatedly to the 'transmitter' and the 'receiver' and to 'aerial electric waves', but this was not his terminology in 1879–80, and if he had referred to the notes he himself made on the very evening of his unhappy demonstration to Stokes and his colleagues, he would have recalled that his own explanation of the phenomena he had discovered was based on 'conduction and nothing else'.

Hughes knew nothing of Maxwell or of the electromagnetic theory of light, but it is clear that he was quite wrong in attributing his results to any form of conduction. Stokes and his colleagues were equally wrong in maintaining that the experiments could be explained as simple induction, but they will have realised that Hughes was a man of very limited scientific knowledge, his apparatus home-made and particularly crude. They were themselves not specialists in the field of electromagnetic physics, and while it is most unfortunate that the weight of their opinion was to have such a discouraging effect on Hughes that he refused to publish any contemporary account of his experiments, they may perhaps be partially excused on the grounds that this was one of those rather rare instances in which a man with little scientific background happens to stumble across a scientific principle of great significance. The fact remains, however, that, had they been a little less categoric and a little less discouraging in a matter on which none of them were authorities, the early history of radiocommunication might have developed very differently.

The story of Hughes's experiments doubtless contains a number of morals, but it is hoped that the above account will disprove any suggestion that Hughes was the inventor of the coherer or that Hughes's work made any practicable contribution to that of Hertz or Marconi. Hughes had no conception whatever at the time that he was dealing with high-frequency electromagnetic waves or that his microphonic contacts were acting as rectifiers of high-frequency current, and I submit that a man who is so lacking in understanding of the nature of the effects that he is demonstrating cannot properly be credited subsequently with the role of inventor. To ascribe to Hughes the invention of the coherer is to deprive Branly and Lodge of the credit that is properly theirs.—Yours faithfully,

G. R. M. GARRATT
The Science Museum, London SW7
11th June 1964

Dear Sir—I was most interested in the letter from Mr. Josephs in the June 1964 *E & P*, p. 210, and eagerly await the promised comments of Dr. Smith-Rose. In the meantime, it may be helpful to point out the difference between the coherer and the imperfect-contact detector.

With the coherer, the filings cohered when influenced by the received oscillations and enabled a current of about 150 μA from a single cell of about 1V to pass and operate a relay which closed a local circuit including the telegraphic instruments. The filings remained conductive until agitated by the tapper, which was connected in parallel with the telegraphic instruments. In cases where reception was by bell, this was mounted so that its hammer tapped the coherer tube, thus avoiding the necessity for a separate instrument.

It is important to note that it was the local battery that operated the telegraphic instruments and quite remarkable that signals could be recorded on tape to give a permanent record at such an early date, and it was not until the arrival of the valve many years later that this again became possible.

On the other hand, the imperfect-contact detector was used with a telephone receiver, and, in this case, it was the rectified received oscillations that gave the audible signal. This provided an extraordinarily sensitive receiver, enabling currents as low as 0·01 μA to be heard when the note corresponded with the natural frequency of the diaphragm, which was usually about 800c/s.

Unfortunately, when using the detector and telephone receiver, it meant that reception had to be by ear, and, although this enabled an experienced operator to select the required signal during interference, it was not possible to record the signal on tape, although experiments were made to operate a very sensitive siphon recorder directly from the detector.

Although Marconi, using a coherer–relay–inker receiver, succeeded in receiving signals on a ship partway across the Atlantic, it was an imperfect-contact detector with telephone receiver that enabled him to hear the famous 'S' signals from Poldhu all the way across the Atlantic. There might have been less scepticism had Marconi been able to record these signals on tape!

Very many combinations of crystal were tested, with and without bias, and it is possible that the early experimenters were unwittingly working on the negative-resistance part of the characteristic, which would account for the exceedingly high sensitivities and, incidentally, for the large amount of atmospherics obtained. It explains also the fact that the detectors were actually made to oscillate—this might even account for 'the generation of radio waves' by Prof. Hughes, mentioned in Mr. Josephs's letter!—Yours faithfully,

E. A. PAYNE
Little Baddow, Chelmsford
15th June 1964

such as red, yellow-green etc., have already crept into the literature.—Yours faithfully,

L. LEWIN
Standard Telecommunication Laboratories Ltd., Harlow, Essex
23rd July 1964

Are engineers educated?

Dear Sir—As Mr. Humphrey implies in his letter (July 1964 *E & P*, p. 227), it is not possible to discuss whether engineers are educated until we have some measure of agreement on what we mean by being educated. We must also decide who are to be included under the title 'engineer', and I suggest that for the purpose of the discussion they should be limited to those qualified for corporate membership of one of the major institutions.

As to education, Alex Waugh, in his book 'In praise of wine', defines an educated man as 'somebody who knows something about everything and everything about something'. This rather neatly sums up much of what Cardinal Newman said and wrote a century ago and which is, fortunately, still read today in the senior forms of a number of schools in 'Newman's idea of a liberal education' (George Harrap and Co.).

One could quote endlessly from this book, but the following passages are especially relevant in the present context:

On 'the primary object of education':
'One main portion of intellectual education, of the labours of both school and university, is to remove the original dimness of the mind's eye; to strengthen and perfect its vision; to enable it to look out into the world right forward, steadily and truly; to give the mind clearness, accuracy, precision; to enable it to use words aright, to understand what it says, to conceive justly what it thinks about, to abstract, compare, analyze, divide, define, and reason correctly.'

On 'the practical end of a university course':
'It is the education which gives a man a clear conscious view of his own opinions and judgments, a truth in developing them, an eloquence in expressing them, and a force in urging them. . . . It prepares him to fill any post with credit, and to master any subject with facility.'

It is this last sentence and its implications which I would particularly emphasise, because I am convinced that the fundamental weakness in modern higher education, and especially in sixth forms, is too early specialisation. A properly trained mind with a bent towards technology would find no great difficulty in mastering any of the subjects mentioned in your editorial comment in the May 1964 *E & P*, p. 139.

Given a broader and more complete basic education as advocated by Cardinal Newman, we should not need to contemplate giving undergraduates of any faculty instruction in the use of the English language, nor should we need to write letters to your journal, Sir, on 'Clear writing' and 'The status of the engineer'.

The answer to the question 'Are engineers educated?' can at present only be that some are, but some are not.—Yours faithfully,

A. W. C. HIRST
2 Vicarage Road, Rugby, War.
1st July 1964

Marconi, Popov, and the dawn of radio communication

Dear Sir—Mr. Joseph's letter in the June 1964 *E & P*, p. 210, prompted me to refer to J. Erskine-Murray's 'A handbook of wireless telegraphy' (4th Edition, 1913), as I vaguely remembered that his book contained a great deal of information on coherers.

On p. 8, Dr. Erskine-Murray states: 'In 1879 Hughes showed that it was possible to transmit signals without the use of a connecting wire, from an induction coil to a microphone several hundred yards away. He rightly believed that the oscillations were propagated through the insulating medium between the instruments, but discouraged by the scepticism of several well-known persons who formed a committee of inquiry, he did not continue his work beyond the experimental stage.' Again, on p. 74: 'The first practical detector of oscillatory currents of high frequency was the loose contact between conductors named by Hughes the microphone, and shown by him to have the power of detecting both mechanical and electrical vibrations.'

I commend this book to anyone seeking information on the early history of the development of wireless telegraphy.

—Yours faithfully,

J. F. PERRIN
6 Cavendish Avenue, Welling, Kent
9th June 1964

Knowing the right people

Dear Sir—Who would have thought that a full member of our worthy Institution could have written such a complacent letter as that by Mr. Raczko in the June 1964 *E & P*, p. 211?

The rules clearly state, and the wording of the application forms agrees, that the Members responsible for proposing and supporting must 'have personal knowledge of his work and character'.

No Member personally knows me, nor many of my colleagues, and I can only assume that this whole process of upgrading is a bit of a fiddle as far as this rule is concerned. If so, why not come out into the open and tell us?—Yours faithfully,

K. WITHAMS
8 Caldew Avenue, Rainham Mark Gillingham, Kent
27th June 1964

[While the number of candidates who have difficulty in getting full Members to propose their election or transfer is small, the problem is a real one for some of those who work in the smaller engineering or nonengineering organisations. In these circumstances the Council are prepared to accept the signatures of Members who have interviewed the candidate personally and made any inquiries that they think are necessary.

It is often possible for the Secretary to put candidates in touch with Members with whom they had contact earlier in their career, and anyone who approaches the Secretary on this matter should give brief details of his experience since he last applied for election or transfer. The Secretary will also provide a list of Corporate members in a particular area where this would help. The situation would, of course, undoubtedly be eased if the many Associate Members who are well qualified transferred to full membership.—Editor]

Why 'Associate'?

Dear Sir—There are a number of cogent arguments to support Maj. Banner's suggestion (July 1964 *E & P*, p. 228) that Associate Members and Members should pay the same membership fees, but I do not agree that transfer from Associate Member to Member should be automatic and merely a matter of time. A considerable number of the present 22000 Associate Members probably could, and should, qualify as full members, but it would be unrealistic to treat them all alike, and unfair to disregard the requirement of superior responsibility which the 5000 present Members have had to satisfy.

The possession of superior responsibility should continue to be recognised by a designation such as 'Fellow', but this should be regarded as a distinction that the Institution confers upon a man, and not something that he has to pay for by way of transfer fees and increased subscription.

The present fees are scaled up from £1 15s. for the youngest student to £5 15s. for graduates aged 35 and over, to meet the practical consideration that the young man cannot afford to pay as much as his elders, and I think this principle should be extended to cover all the Corporate members, with one, two, or possibly three steps above the age of 35. I am confident that a rational fee structure of this kind, related basically to age, could be introduced, and would preserve the Institution's income without imposing serious hardships on individuals. The group principally affected would be the Associate Members, where younger men would pay less, and older men more, than the present fees. This would not be unreasonable, and it would, of course, completely remove the existing disincentive to transfer to the highest class for which a man is qualified.

Since my letter, which originated this discussion, appeared in *E & P* last October, you have published about a dozen letters, all broadly in support, and no dissentient voices. I hope this evidence of feeling from the membership will strengthen the hand of our Council in the EIJC discussions, and enable the Institution to make progress on this much overdue reform.

—Yours faithfully,

D. J. SAYERS
Oak Gates, Cuddington Way Cheam, Surrey
13th July, 1964

* July 1964 *E & P*, p. 230, col. 1, line 6

Letters
TO THE EDITOR

Marconi, Popov, and the dawn of radio-communication

Dear Sir—I was interested to read the letter by Dr. Smith-Rose in the August 1964 *E & P*, p. 276. That Oliver Heaviside was present at the meeting of the 28th January 1886 can be verified by consulting the documents in the Heaviside Collection of the Institution. Since Heaviside was an Associate Member who had served on the Council, the fact that he attended this meeting cannot be regarded as an unusual event. But the fact that his attendance was not recorded in Vol. 15 of the *Journal*, as Dr. Smith-Rose points out, calls for an explanation.

In 1885, the Secretary, Mr. F. H. Webb, wrote to Heaviside and told him that unless he paid his subscription his name would be placed on the black list. Heaviside replied that he was unable to pay at present, but would do so at the first opportunity. I have searched through the Heaviside papers but have found nothing to indicate that he had paid his 1885 subscription. So it is quite possible that his name was on the black list in January 1886. Then, in accordance with the custom at that time, his remarks would not have been recorded in the *Journal*. In any case, the recording procedure for the *Journal* in 1886 was rather arbitrary and left much to be desired.

Although Heaviside's views of the 1886 meeting were not recorded in the *Journal*, they were definitely recorded in letters to Prof. Oliver Lodge and Dr. G. F. C. Searle. These letters were exhibited at the Heaviside Centenary Meeting of the 18th May 1950. For a published account of Heaviside's views, reference should be made to his 'Electrical papers', Vol. 2, p. 28, *et seq.* In reading this account, it must be remembered that Prof. Hughes did not describe his experimental results in the generally accepted language associated with Maxwell's theory. He used the language appropriate to his own intuitive topological concepts of electromagnetism.

It was shortly after the 1886 meeting that Heaviside began his unkind criticism of the 'peculiar' language and grammar used by Prof. Hughes. Some of the details will be found in *The Electrician* of 1886. Hughes ignored the cocksure attacks inspired by Heaviside; and his silence led his critics to assume that his radio researches were unsound. Prof. Hughes was not a professional physicist. He was a rich man who had retired and had made physical research his hobby. The professional physicists of his time, being unable to understand his abstract topological habit of thought, found it easy to regard him as an amateur. Hughes, however, appeared to be indifferent to their opinions. The result is that today he has the sour distinction of being the most underrated physicist of the 19th century.

The letter from Dr. Garrett (August 1964 *E & P*, p. 276) shows that in 1880, when Prof. Hughes demonstrated his wireless-signalling experiments to Mr. W. H. Preece (the Engineer-in-Chief of the GPO), he received no official encouragement. Apparently Preece showed little interest in the equipment and did nothing to help Hughes. But Preece's attitude towards wireless signalling must have changed considerably in the years that followed 1880. After developing his own system in 1896, he invited Mr. G. Marconi to bring his equipment to England. The Post Office engineers who examined the Marconi apparatus found that it was home-made and rather crude. Furthermore, they found that none of the individual appliances were novel in themselves. The sending and receiving appliances did not differ materially from known forms of Hertzian oscillator and detector; while the coherer was of the type invented by Hughes. What was new was Marconi's patented plan of joining these appliances in a circuit containing an elevated iron pot and a connection to earth.

The Post Office engineers subjected the Marconi apparatus to a number of tests, and certain modifications were found to be necessary. Pressed forward by Preece, the work of modification and construction was finished by the end of April 1897. Masts 112ft high and capped with large galvanised-iron drums had been erected at Lavernock Point (near Cardiff) and on the island of Flatholm (in the Bristol Channel). Early in May, tests were commenced. The experimental results provided conclusive evidence of the practicability of wireless telegraphy.

Preece announced the success of the experiments, and, owing to his powerful support, Marconi's plan of wireless telegraphy became a matter of great public interest. Glowing articles on Marconi appeared in all the national newspapers. Almost everywhere he was acclaimed as the inventor of wireless telegraphy.

A financial syndicate was formed, and Marconi returned home with £60000 in his pocket—the founder shares of the Marconi Wireless Telegraph Co.—and with the Lodge–Marconi tuning patent, which was worth ten times more than his original patent.

All this shows that a scientific historian who wishes to write on the subject of the 'inventor of radio' will not have an easy task. For he must try to understand how the young Irish-Italian Marconi, who came to England in 1896 with a half-baked patent, was suddenly transformed into a wealthy world-famous figure. And in seeking this understanding he will probably experience a transient feeling of regret that Prof. Hughes failed to get Post Office backing in 1880.—Yours truly,

H. J. JOSEPHS

'Homefield', Clover Rise, Whitstable, Kent
24th July 1964

Marconi, Popov, and the dawn of radio-communication
H. J. Josephs, M.I.E.E.

Technician engineers
H. G. Sims, A.M.I.MECH.E., A.M.I.E.E.
B. Dawkins, B.SC.(ENG.), A.M.I.E.E.

Wrapping IEE publications
K. F. Shrubb, B.SC.(ENG.), A.M.I.E.E.

Status of the engineer
R. F. Groom, GRADUATE I.E.E.

R.M.S. (or mean) volts
T. Palmer, B.A., GRADUATE I.E.E.
A. Felton, B.SC.(ENG.), M.I.E.E.

Cosmic electrical discharges—16
C. E. R. Bruce, M.A., D.SC., M.I.E.E.

Guidance for the fog-bound motorist
R. M. Robbins

A new deal for vector diagrams
D. A. Hopkins, B.SC., A.M.I.E.E.

High-voltage load control
P. Hirschmann, B.SC.(ENG.)

Inverse-square law in the theory of relativity
Prof. J. Palacios

Librarian's nightmare
W. Dougharty

Power factor—are our definitions good enough?
R. Wells, M.I.E.E.

Phasors and vectors
D. Roddy, A.M.I.E.E.

Why 'Associate'?
W. E. Doran, M.A., A.M.I.E.E.

Discharge detection and measurements terminology
G. H. Hickling, B.SC., M.I.E.E. and R. Hawley, PH.D., A.M.I.E.E.

Interpretation of the cosmological red shift
Prof. R. O. Kapp, B.SC.(ENG.), M.I.E.E.

Technician engineers

Dear Sir—It seems a pity that a place could not have been found for the technician engineer within the structure of the Institution of Electrical Engineers. The Institution would have benefited financially, and the wealth of practical experience which these engineers are continually gaining would have been available. Instead, there will be separate meetings and publications, and the IEE papers will continue to be highly specialised and not of sufficient general interest.

It is unfortunate that the title chosen for the new Institution has a similarity to that of the Institution of Electrical Engineers when used in abbreviated form. The initials of both Institutions will include I.E.E., and this will be a retrograde step in the fight